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Reviewed work(s):
Source: *The Journal of Wildlife Management*, Vol. 14, No. 2 (Apr., 1950), pp. 105-114
Published by: Allen Press
Stable URL: http://www.jstor.org/stable/3796316
Accessed: 29/08/2012 11:07

THE JOURNAL OF WILDLIFE MANAGEMENT

VOLUME 14 APRIL, 1950 NUMBER 2

PROBLEMS AND NEEDS IN PHEASANT RESEARCH*

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A mid-century appraisal of our professional competence to deal with small-game management problems would indicate important regional progress for several species. We probably know at least a few things that can be done in some areas to have more quail, more rabbits, and more squirrels.

Applying this same test to the introduced pheasant, however, gives little cause for satisfaction. Our knowledge and skills are inadequate to the job we are called upon to do. The measures commonly relied upon for this species might be called "desperation management" and, whether deliberately or not, they are in use for want of something better.

It is true we know how to produce more food for a local flock of wintering birds. We can keep a few cocks safe from the hunter with posted refuges. Additional cover will help to foil both hunter and fox, and predator control by direct attrition may preserve some birds at least seasonally. But the country's best pheasant range is producing without such expedients and their use in marginal range has yielded little if anything in year-to-year increases. Evidently we are not getting down to real "limiting factors."

Of course, we can side-step the management problem completely on a small scale by rearing birds on game farms and turning them out immediately before the season on small intensively hunted areas. Under these conditions we may bring 50 to 80 per cent of the cocks to bag. For one day this may satisfy the individual gunner, although by ordinary standards such sport rates rather low as outdoor recreation. Actually, there is nothing wrong even with a shooting gallery, as long as the individual participants pay the full bill for what they get. We can question whether it is sound to use the license fees of several hunters to give temporary satisfaction (as contrasted with really good hunting) to one individual when all might be benefited by spending the money in another way.

Here the research man should step in and tell us how the money should be spent. He probably can do a fairly good job if the entire wildlife program is considered. But in most areas, getting pheasants for those funds is beyond us at present. Considering the needs of "action" programs, it is difficult to find anything anywhere to cite as a

* Presented at the Northeast Wildlife Conference, Syracuse, N. Y., Feb. 3, 1950.

reliable and proved technique which will produce more birds at a fair price. This in spite of the fact that at least 25 states now have research projects on the pheasant (Wandell 1949). It is appropriate to ask: "What are we learning?"

In studying this bird, the biologist has the frequent and disquieting experience of having to admit that some of the most significant things he "knows" about it—are not completely true.

This species, we say, is a bird of the north—yet it recently has become established on irrigated lands in New Mexico at a latitude near that of Savannah, Georgia. But even this is not the southern limit. There are ringnecks south of the Rio Grande and in Baja California. The Schwartzes (1949) describe a wide variety of conditions occupied by pheasants in the islands of Hawaii, from sea level to 10,000 feet and from tropical areas to regions of freezing temperatures.

Pheasants thrive best on glaciated soils —or do they? Some of the largest per-acre yields of this bird now are being taken on the unglaciated ricefields of California's Sacramento Valley (Ferrel, Harper, and Hiehle 1949).

The Chinese ringneck is associated with agricultural lands, and particularly grain-farming areas. It is a bird of fertile soils. And although these things are characteristic, again there are exceptions: Fairly respectable populations are found in relatively infertile and uncultivated parts of the Nebraska Sandhills (Sharp and McClure 1945), and there are birds in the forest and range lands of Hawaii.

We need a broad view of the pheasant. Studies that do not see beyond the borders of a state can easily go wrong. On the other hand, there are important differences between regions. Fifteen years ago a widespread impression existed that winter survival was a big problem in eastern pheasant management. Food patches, winter feeding, and cover plantings were prescribed as the key to better hunting. Today, in the region between Chicago and Boston, you will hear little talk about such methods, and little indeed about starvation or winter-killed pheasants. These resourceful birds have been watched through too many winters by too many critical observers.

But proceed west from Lake Michigan into that land of rich unleached soils where the buffalo roamed and where Dakota grainfields now form the nation's best *extensive* pheasant range. Here prairie blizzards point up the reduction of cover to its irreducible minimum and starvation losses become a convincing reality.

It seems that practically all our generalizations fail to describe the pheasant accurately because the species itself is generalized. The Chinese ringneck is widely successful because it is a natural cosmopolite and we are not likely to appreciate its full possibilities until someone has gathered notes and eggs across Asia on the trail of Marco Polo from the Caucasus to Formosa.

What happened to pheasant populations during the forties, on almost a national scale, certainly highlights our ignorance of this bird in particular and some wildlife fundamentals in general.

On a basis of past experience, it was reasonably predictable that the early forties would bring another periodic reduction in certain native birds and mammals. It came in a degree that was unmistakable, and for most areas 1942

and 1943 were the most eventful years. By 1944 it was evident that widespread declines had occurred in hares and rabbits and native grouse. It is significant to note that, beginning perhaps a year or two earlier, there were extensive reductions in the numbers of common birds and mammals in northwestern Europe, including such species as black game, capercaille, willow grouse, hazel grouse, (Hungarian) partridge, pheasant, and varying hare (Siivonen 1948).

In this country the Hungarian partridge had previously shown a tendency to participate in the population rhythms of native species. That the pheasant would get into the "swing of things" in the forties was not, to my knowledge, specifically anticipated by anyone.

In the Midwest, at least, it did just that. On areas under my own observation in Michigan, 1941 was an exceptionally favorable year for both pheasants and rabbits. Both were plentiful in 1942, although pheasants were reduced from the peak of the year before. It is interesting and perhaps significant that Grange (1949, p. 122) observed the same trend in Wisconsin ruffed grouse. In 1943 conditions appear to have degenerated for both pheasant and grouse over a wide area.

This important phenomenon occurred in the midst of the war at a time when research projects were being radically curtailed. Nevertheless, enough investigators were on the job so that, if truly *effective* observational and sampling techniques had been in use, it should have been possible to establish beyond question exactly what happened to the pheasant.

Although there is no precise proof on this point, there does seem to be a fairly general agreement that a persistent unfavorable trend in spring weather occurred for several years in widely scattered areas and that this is the one *observed* factor which might have caused such a widespread reduction. As would be expected, observations in some localities implicated weather more clearly than in others (Allen 1946, 1947; Carlson 1946; Einarsen 1946; Ginn 1948; Perry 1946), although poor nesting success and unfavorable age ratios were reported from nearly every region where pheasants were being studied (Bach 1946; Faber 1946; Hendrickson 1944; Kimball 1948; Leedy and Dustman 1948; Lundy 1946; Mohler 1948; Nelson 1948). Published observations are borne out by the results of Wandell's (1949) questionnaire and the records gathered by Kimball (1948) through correspondence. Impaired reproduction is agreed upon and suggestive weather trends are prominently mentioned.

Suggestive also is the fact that during the years of the decline, when spring weather tended to be wet, or wet and cold, in many localities, the center of distribution for South Dakota pheasants shifted westward toward the dryer part of the state (Kimball 1948). On a trip to Colorado in 1948 I tried by personal inquiry to determine whether there had been any decline of pheasants in the irrigated area of northeastern Colorado where wet spring weather would not be a factor. I could find no one who had heard of a decline in that area, and Wandell (1949) evidently got a similar reply from the state. He brings out the fact that recovery from the slump has been most rapid on the west coast, and we might add that a similar resilience has been evident in

the birds on Pelee Island in Lake Erie where a complete return to abundance was attained in 1948 and 1949.

In discussing weather, it should be mentioned that Cartwright (1944) definitely ascribed the drastic reduction in 1942–43 of sharptailed grouse and Hungarian partridges in the prairie provinces of Canada to below-normal temperatures and above-normal precipitation during the hatching period, which suggests that the synchronism among farmland pheasants, plains inhabiting sharptails and Hungarians, and forest dwelling ruffed grouse is not a matter of chance. Waiting to be included also, of course, are the mammals. Grinnell's (1939) observation of a wet-year reduction in California small mammals indicates that this is a reasonable possibility.

It is logical to suspect that these large-scale decreases are related, and if they are, then the total phenomenon is not covered adequately by Grange's (1949) penetrating but necessarily incomplete explanation of cyclic mechanisms. Pelee Island pheasants, for instance, were not affected by a sudden radical change in plant successions, agricultural operations, or in predation pressure. Yet they declined at the same time as bird populations far removed (Clarke 1947; Stokes 1948). It can hardly be doubted that the fundamentals discussed by Grange, including the cyclic nature of weather trends, certainly must be involved in the population rhythms of living things, and his book is an important step toward understanding these primal behaviorisms.

If the decline of game birds and/or mammals was a direct effect of adverse spring weather, it does not appear to be unique, as wildlife literature contains numerous references to such occurrences. In American Game for 1926 the following note appears:

"Thirty years ago, partridges [Hungarians] in England were not doing well. Owing to a succession of seasons in which the Spring of the year was attended by rough, boisterous weather preventing the successful rearing of the young birds, the coveys in most parts of the country had become few and small" (Hunting 1926).

These records which appear to involve weather in the recent pheasant decline have not been cited in an attempt to prove anything. Rather the entire discussion leads to the inescapable conclusion that from coast to coast something big and fundamental happened in the forties not only to the pheasant but to many species of birds and mammals of economic importance. Yet, since the termination of the New York grouse studies in 1942, not a single adequate *research team* has been organized to follow the fate of a species known to be cyclic. It is obvious, of course, that there was no pheasant study intensive enough to identify the specific factors involved in the series of spectacular crop failures. Our impressions of weather probably are significant, but they fall far short of scientific exactitude. There were years and areas where pheasants reproduced satisfactorily in spite of what appeared to be adverse weather. It is evident that we lack a factual concept of the limits within which combinations of climatic variables are important to pheasant reproduction.

We are in the hypothesis stage of development in learning about weather and pheasants. In 1940 Bennitt and

Terrill advanced a suggestion that the southern limit of pheasant distribution in the Midwest might be determined by ground temperatures which could not be tolerated by eggs exposed to the sun. Eight years later, with the same idea in mind, Graham and Hesterberg (1948) drew climatographs (temperature plotted against precipitation) of areas where the species had succeeded. They found that "one segment of all the graphs of localities where the birds have been successful fall in the same area. Between the first of April and the first of June . . . birds experienced similar conditions in all successful localities studied."

Is this significant? And does it relate significantly to effects of weather periodicities during the April-June nesting season in areas where pheasants ordinarily thrive? Thoughtful hypotheses like those cited should be the forerunners of intensive research projects—but are they? How much money is being spent in getting at the elemental facts of our pheasant-climate enigma?

That money is being spent on this species there can be no doubt. In 1930 a summary of stocking activities for the nation indicated that 31 states liberated some 174,000 pheasants (Anon 1930). Eighteen years later, another survey showed that 32 states had stocked roughly $1\frac{1}{4}$ million birds (Pushee 1948). It is likely that in two or three individual states the yearly expenditure for pheasant management (sic) now approaches or exceeds half a million dollars, and for the entire country artificially stocked birds probably represent a total application of land, labor, and capital exceeding $2\frac{1}{2}$ million dollars. The annual crop of ringnecks is getting attention, and it is a big operation as wildlife operations go. It has a significant relationship to research accomplishments or the dearth thereof.

Ordinarily we conceive of research as being that small-scale trial and error which tests and proves production methods before they are applied in big expensive programs. However, this is a purely theoretical concept as applied to pheasant activities. The research man has consistently found himself tossing like a chip in the backwash of big stocking programs grabbing at a fact here and an indication there in an attempt to find out what was being accomplished. When he had found out, or thought he had, there was no one to listen, for likely enough the administrator who hired him was so busy bringing fulfillment to the action-in-our-time philosophy of a demanding public that the question of *whether* to stock never reared its ugly head.

For anyone with the inspiration to search it out, there is a fairly adequate literature on artificial stocking, although most of the research has been extensive rather than intensive. To a biologist the evidence is convincing that such one-by-one methods are costly and inefficient and that they offer little possibility of achieving real satisfaction for any appreciable segment of the hunting public. Further, it appears that the regular dissipation of funds in this manner delays indefinitely the land management job by means of which the tremendous natural productive capacity of wild populations can be employed to get results on a scale that will really count.

To the biologist all this seems evident, but it is not evident to a public

conditioned to the assumption that all blessings flow from assembly lines and that enough pressure will accomplish anything. Whether or not they live in what can reasonably be considered pheasant country, there is a tendency for some groups to confront their state administration with the alternative of ringnecks or wrung necks, in which situation it is obvious that the most comforting thing a research man could do would be to find out why what the public wants is right. Sadly for the administrator, he seldom gets this comfort. Such a situation is unfortunate, for neither creative art nor creative science can flourish in an or-else atmosphere.

The further research called for by the artificial stocking issue probably is mass tests similar in scope and execution to the project carried out for three years on the Sartain and McManus ranches in California (Ferrel, Harper, and Hiehle 1949). If the results of large-scale controlled experiments are consistently publicized it seems likely that a sufficiently large portion of the public can be convinced of biological truths so that research can become an effective guide to policy. The failure to interpret findings realistically or to publicize facts that run contrary to existing operational procedures probably accounts in part for the regular recurrence of unsound public demands.

It is well established in wildlife science that animal populations are productive only in favorable environments. Controlling conditions on the land, especially in terms of the vegetation pattern, undoubtedly is our best key to management. Yet it is an unfortunate fact that pheasant requirements are so poorly understood that for most areas land improvement specifications simply can not be drawn. The research man may prove that artificial stocking does not pay, but he is in the somewhat unsound position of not being able to furnish a workable substitute. This is not completely so everywhere, since cover certainly is one obvious need on the prairies, but for most of the nation's pheasant range there is little that can be advocated with assurance.

It is consistently true, I believe, that *large* pheasant populations are produced only on fertile soils and that practically always they have access to the crops and early plant successions of cultivated areas. If these are requirements, they are difficult to build to order. We may come to a future acknowledgment for this species that good habitats largely exist but are not made.

The reasons *why* pheasants thrive on fertility and *why* they are restricted to certain regions are basic questions that research of the type done in the past 20 years has hardly begun to answer. Investigations into the physiology, nutritional requirements, breeding intolerances, and other inherent characteristics of the organism will provide a basis for interpreting the more superficial data now being gathered. State surveys, kill records, sex and age ratios, brood counts and food habits studies all are necessary and significant, but a much more fundamental knowledge is essential to an understanding of what counts in pheasant management.

We have had a scattering of exploratory and specific studies in the field of pheasant physiology, among which can be mentioned the experiments begun by Gerstell (1938) and Latham (1947)

in Pennsylvania, those of Long (1948) in New York, Shick (1947) and others in Michigan, and the investigations of Kirkpatrick (1944), Kabat, Buss and Meyer (1948), Thompson and Baumann (1950), and others in Wisconsin. No one has yet defined the minimum nutritional requirements of pheasants, and there are critical gaps in existing information on reproduction.

As suggested earlier, excellent use could be made of more detailed data on the habits, affinities, and regionally adapted varieties of pheasants on their native continent. It is entirely possible that there are other stocks than those we now have which could extend the range of the bird in this country. However, the best approach to this is to determine just what factors are responsible for confining the species and then find a solution after we know what the problem is.

What I am urging specifically is that more "pure science" be infused into the pheasant research program. A welter of relationships must be unraveled, and many issues will require controlled experiments on captive birds. Some such analytical work will be long-term in character. The application of facts will not be immediately evident. It will be unspectacular. But we are waiting for it, and agencies concerned with the production of annual crops of pheasants can well afford to carry a limited amount of such work along with the empirical cover-and-food type of study that gets a more ready public acceptance.

After a slow start, the population dynamics of pheasants are getting gratifying attention. Refinement of techniques is of particular importance and several excellent recent advances have been made. The reference tables for dating nesting events as constructed by Thompson and Taber (1948) should be widely useful. Kimball (1949) has described a type of crowing cock census that probably will replace the relatively inefficient roadside census as a means of getting extensive population indices. Island research has been particularly productive of significant population information. The work of the Oregon Wildlife Research Unit on Protection Island and Eliza Island has been notable (Einarsen 1945; Scott 1948) and on Pelee Island Stokes is gathering what will undoubtedly be the most revealing set of population statistics that has yet been available for study. This investigation at Pelee should be kept going indefinitely, since the island certainly is the most rewarding pheasant laboratory yet discovered.

The extent to which it is possible to elaborate and intensify pheasant research will depend upon the extent to which it is given necessary economic support. The administrator who wants imaginative and productive research will need to provide for the continuance in such work of able men with long training. This means paying for technical knowledge and research ability on a scale comparable to that of administrative responsibility. A few such positions give continuity to investigations and insure proper training for subordinates. Incompetent research is considerably worse than no research at all, since it embarrasses sound programs and spreads confusion far beyond its own bailiwick.

Since so many states now have pheasant research in progress, there are mani-

fest possibilities for overlap and unnecessary duplication. The need for more rapid exchange of information and the desirability of coordinating investigations nationally have been discussed at recent regional meetings. At the North American Wildlife Conference in 1948, Kimball (1948) called attention to " . . . the need for a coordinated regional pheasant research program wherein theories and ideas would be correlated and techniques of obtaining uniform basic data would be developed and standardized."

Most of the present discussion has hinged on the activities of states, and with good reason. The federal government has had no pheasant research program as such. It is with a realization of this deficiency and a recognition of the need for regional studies that Fish and Wildlife Service gamebird investigations now are being enlarged to include the biology and range requirements of pheasants. The leader of this project should be able to bring to research on this species something it has largely lacked heretofore—a nationwide perspective. He will keep in close touch with work being done in the various states and design his own research to fill in the gaps. Entirely on an informal basis, he will attempt to promote cooperation and a more rapid exchange of information among workers in this field.

During the past two decades, funds allocated to pheasant research have been far from commensurate with total expenditures for this species. The total effort has not been great, and much of the work has been done by young men just beginning their careers as wildlife biologists. As their experience grew, they were lost to research because their economic necessity had also grown. This probably is the most important reason why we have not had more progress in investigations of our particularly difficult pheasant problems.

As of the present, we know that the ringneck can tolerate a wide range of conditions, and we have not yet learned what really is essential. This ignorance is reflected directly in the primitive state of applied management. In terms of the progress now expected in any branch of scientific endeavor, it is doubtful whether we are more than 20 years removed from the measures practiced by Kublai Khan. Except for the advent of the electric incubator, our status would be in even greater question, since Kublai Khan had one major advantage: He was in a position to manipulate the supply of hunters at least as easily as the supply of game.

If we were to formulate a "10-year plan" for the cooperative improvement of pheasant research on the part of all agencies, state and federal, it probably would involve the following points of departure:

(1) Administrative recognition of the complexity of pheasant management problems and the need for supporting a high-quality sustained research program.

(2) Where possible, studies should be carried out on a *research team* basis. This would include well-trained and seasoned supervisory personnel and at least part-time service by specialists as need arose.

(3) The inclusion of long-term basic studies on physiology, nutrition, and reproduction, and sample-area experimentation for the improvement of habitats.

(4) Regional conferences for all personnel in pheasant work, to be held annually with at least one full day devoted to discussions of research and management.

(5) Interstate travel by field personnel to permit inspection of projects, comparison of techniques, and cooperative planning of programs.

(6) Free interchange of reports and publications among all agencies concerned.

Such an approach is suggested only for the federal government and those states with sufficient resources and with a sufficient stake in the management of this species. With participation by all interested agencies it is likely that 10 years of concerted effort would bring about more effective and realistic management in areas that can produce pheasants. A 10-year period would not produce the answers to all problems, but it might be the proper interval for reappraisal and, perhaps, for the publication of a book summarizing progress.

This review of the pheasant program probably can serve as a critique of wildlife investigations in general. In no other field of science have we taken research so lightly. Basically, this probably is an important reason for evidences of amateurism, inattention to fundamentals, and the want of a sound management philosophy. Immaturity can, perhaps, only be cured with time; but the process surely will be speeded by facing the issues and going to work on them.

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